which call for more careful and precise statement, but on the whole the work is calculated to be useful, and we can recommend it for school use.

Glimpses of Feverland: or, A Cruise in West African Waters. By Archer P. Crouch, B.A. Oxon. (London: Sampson Low, 1889.)

IN this volume Mr. Crouch presents a record of the impressions made upon him by the land and people of certain portions of West Africa, which he visited in connection with the laying of a cable to put various places, principally French and Portuguese, in telegraphic communication with Europe. A large part of the book is devoted to an account of what he saw during his passage from Accra, on the Gold Coast, to the Portuguese island of St. Thomé. Afterwards we have a full description of St. Thomé and St. Paul de Loanda, and in several concluding chapters the author sums up the incidents of his voyage homewards. Mr. Crouch is so good an observer, and has so frank and lively a style, that his narrative, taken as a whole, is fresh and interesting, although his subject is often, apart from his treatment of it, dreary enough. He is particularly successful in those passages in which he seeks to give his readers glimpses of native customs and superstitions. It is worthy of note that he has formed a very unfavourable judgment as to the intellectual and moral qualities of the Negro race; but on this question, with regard to which he differs widely from Mr. Stanley, he perhaps speaks rather more positively than the extent of his experience warrants.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Meteoritic Theory.

I HAVE during the past six months been led from the study of our own atmosphere to consider certain phenomena relating on the one hand to the solar *atmo-physics*, and on the other hand to the evolution of our own globe and its atmosphere. There has thus arisen in my mind a system of cosmogony which has led me, quite independent of Mr. Norman Lockyer's published course of reasoning, back to a meteoric theory that will, I hope, be acceptable to yourself and others. Awaiting the preparation of these views for publication, I have had occasion to look over the report on the total eclipse of the sun, July 1878 (Professional Papers of the Signal Service, No. 1, Washington, 1881). I quote from pp. 49 and 50 some paragraphs to show the connection between views then held and those at which I have recently arrived.

Washington, April 29. CLEVELAND ABBE.

"Under these circumstances, I could but regard the suggestion that occurred to me on July 29 as a slight but important extension and modification of the views previously held. . . . It amounted to saying that a large part of the phenomena of the outer corona is essentially non-solar, having to do with cold meteoric matter that is beyond the solar gaseous atmosphere and is shining by reflected light, . . . rushing on its way to plunge into the sun's atmosphere, where, within a few hours, it would be dissipated. . . . That these beams were due to wholly new meteor streams . . . now for the first time approaching the sun. . . . I am now inclined to extend this view to very many of the radiating dark and bright lines observed during eclipses, and would explain most of them as due to brightly illuminated groups and streams of meteors and to large meteors followed by trains. . . . Those meteors that enter the solar atmosphere and become incandescent will of course shine with a greatly increased splendour, and thus constitute a portion of the inner corona; these thus show us the limit of the gaseous atmosphere of the sun. . . . The extreme limit may be located at a distance of five minutes (of arc) above the sun's surface, and is very likely to be less than this. . . . Meteors glow as shooting-stars when they strike our atmosphere with a relative velocity of from twenty to forty miles per second, and at an altitude of about one hundred miles, where the density of the atmosphere may be about 3×10^{-9} times that which prevails at the surface. Now these same meteors will, when they have approached to within 130,000 miles of the sun's surface, have a momentum at least a hundred times greater than that with which they enter the outer limit of the earth's atmosphere; therefore we are allowed to assume the density of the outer limit of the supposed solar atmosphere to be but the hundredth part of that of the earth, or 3×10^{-11}; this gives us for the base of the solar atmosphere a density and a pressure quite within plausible bounds."

The Structure and Distribution of Coral Reefs.

THOSE who have read the additional appendix in the edition just published of Mr. Darwin's work on coral reefs will doubtless have observed that whilst the recent evidence there produced against the theory of subsidence lies chiefly in observations on living reefs in the Florida seas, in the Western Pacific, and in the Indian Ocean, the new testimony advanced on behalf of the theory is in the main indirect in bearing, and is based on assumptions that have yet to be proved.

Referring to the latter evidence in the order of mention, I come first to the 90-fathom reef off Socotra, a reef that is assumed to have been lowered by a movement of subsidence into its present position. So defective is our knowledge of the depths at which coral reefs may grow, and so incomplete is our acquaintance with the complex agencies that combine to produce a coral reef or to limit and prevent its growth, that the inference respecting the depth of the Socotra reef may be truly characterized as based on an unproved assumption. At present our acquaintance with the fauna of the submarine slopes of tropical islands in the Indian and Pacific Oceans, between the depths of 20 and 100 fathoms, is of the scantiest description; and we are not in a position to hazard even a guess on the subject, much less to assume that an island like Socotra has experienced a movement of subsidence because it possesses a reef "submerged in some places to a depth of over 90 fathoms." It is owing to our ignorance of the fauna in these depths that it has not been possible to identify the great numbers of minute molluscan shells, which occurred in the upraised post-Tertiary muds discovered by me in the Solomon Islands; and it is of the lack of such knowledge that Prof. K. Martin in his recent great work on the Tertiary formations of Java naturally complains. Surely, when the biologist is fain to acknowledge his want of acquaintance with the matter, and when as a consequence the palæontologist and the geologist have to bring their labours to a standstill from the lack of comparative material, it seems rather dangerous for the coral reef speculator to assume what has never been properly examined.

If, therefore, we have yet to determine the limit of depth of the growth of coral reefs, we are scarcely in a position to advance as evidence in behalf of subsidence the thickness of certain upraised beds of coral limestone in the West Indies and in the Sandwich Islands. Even if such evidence should be ascertained to be valid in itself, it must be remembered that Mr. Murray in his theory places no limit to the possible thickness of coral reefs, and that in the outward growth of a reef a considerable thickness may be produced. There was, in truth, no circumstance more impressed on my mind in the Cocos-Keeling Islands than the seaward extension of coral reefs.

Once more, however, I would impress on future investigators the extent of our ignorance of the depths in which coral reefs may form. In one of my papers (Proc. Roy. Soc. Edin., 1885-86, p. 887) I have pointed out that the estimates of observers in different regions vary between 5 and 40 fathoms. It is also singular how different observers may vary in the conclusions they draw from the same lines of soundings. The soundings off Keeling Atoll were made by Captain Fitzroy himself, and he places the limit of depth at 7 fathoms ("Voy. *Adventure* and *Beagle*," ii. 634); whilst his companion, Mr. Darwin, judging from the same soundings, concluded that it lay between 12 and 20 fathoms. In truth, as long since pointed out by Prof. Semper, the whole question of the depth of the reef-coral zone has never been methodically investigated. It never occurred to Mr. Darwin or Prof. Dana that coral reefs might grow in depths *beyond the belt of sand that apparently limited their growth.* Yet

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such I found to be actually the case on the slopes of Keeling Atoll, where Captain Fitzroy's soundings were taken. Few, in fact, who have read the work of the young naturalist of the *Beagle*, can sufficiently realize how scanty were the data on which the fundamental inference of the theory of subsidence was based; and I may safely add that few must be the scientific questions that have been settled on a scantier basis of observation than that relating to the depths at which reef-corals grow.

With regard to the sections exhibiting the submarine profile of the reef of Masámarhu Island, I was first at a loss to understand why their character should be advanced as favouring the idea of a movement of subsidence, since, when they first appeared in NATURE (vol. xxxvi. p. 413), I regarded them as favouring Mr. Murray's views. However, it soon appeared that this opinion was based on our inextensive acquaintance with the habits of corals, especially with the limiting causes of their extension in depth. The "ditches" shown in these sections I look upon as indicating the formation of barrier-reefs at considerable depths, and as giving remarkable support to my views on the origin of these reefs. In the paper above quoted I have been led by the observations of Agassiz in the Florida seas, and by my own in the Solomon Islands, to the conclusion that the main determining condition of the depth in which reef-corals thrive is to be found in the injurious effect on coral growth of the sand and sediment produced by the breakers, and that the distribution of these materials is dependent on the angle of the seaward submarine slope and on other less important circumstances.

It follows from this that in those localities where the submarine slope is moderate, a barrier-reef will form beyond the belt of detritus derived from the shore-reef inside it. But when the slope is fairly steep, as in the case of Keeling Atoll, the reef *débris* will cover an area of much less horizontal extent, and, as at these islands, an off-lying line of coral shoals will spring up at a distance only of 150 or 200 yards from the inner reef. Should, however, the slope be precipitous, as at Masámarhu Island, the reef *débris* will extend to considerable depths; and beyond the area thus covered with sand and gravel, a line of reef will in the course of time grow upwards, giving rise to the so-called "ditches" of the sections.

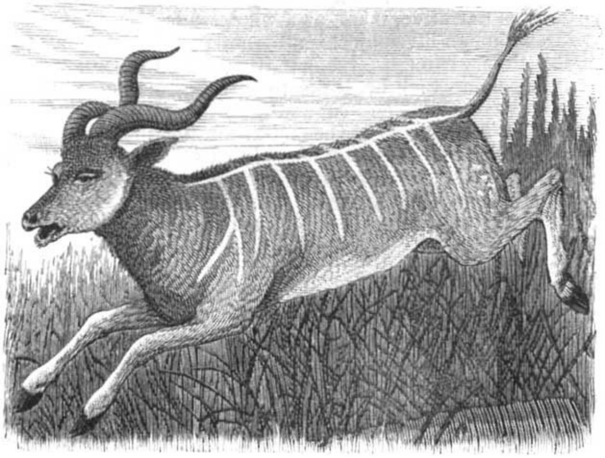
Reefs of all classes, as I hold, have a two-fold mode of growth seawards. There is first the advancement of the outer edge of a reef on its own talus, as dwelt upon by Mr. Murray. In the second place, they grow seaward by a reclaiming process, whether they be fringing, barrier, or circular reefs. The distribution of the sand and *débris*, guided by the angle of the submarine slope, determines, as above shown, this second mode of growth, which may result, as at Keeling Atoll, in a line of adjacent coral banks that ultimately reclaim a new strip and add it to the width of the reef, or a more distant barrier reef may appear at the surface, the lagoon of which silts up and is choked with coral in the course of ages, or, as at Masámarhu Island, there may occur a deeply-submerged barrier-reef that can be discovered only by methodical soundings.

Let us take the instance of Keeling Atoll to illustrate the present condition of this controversy. In a series of papers on this celebrated atoll, that I am preparing for the Royal Scottish Geographical Society, I have shown that the direct evidence of subsidence adduced in its case by Mr. Darwin is, according to Mr. G. C. Ross, its present proprietor, founded in error. In truth, Mr. H. O. Forbes, during his visit in 1878, observed evidence leading in his opinion towards a movement of upheaval. In default, then, of direct evidence, we have to look for the indirect proofs to certain *a priori* considerations, based on a few soundings that appeared to demonstrate once and for all the limit of depth of the reef-coral zone, a subject concerning which we still have very incomplete data. Then we are transported across the Indian Ocean to the 90-fathom reef of Socotra, the origin of which, for the reason just stated, is hidden in mystery. Afterwards, appeal is made to the thickness of reef limestones in Cuba and in the Sandwich Islands, limestones which, it is alleged, could only have been formed during subsidence, notwithstanding that their exact nature has not yet been determined, and in spite of the circumstance that reefs can attain a considerable thickness, as Mr. Murray rightly holds, solely by their outward growth.

If a visitor from another planet, having thus far followed the discussion, were to inquire in an apologetic manner whether, instead of going to the other hemisphere for evidence we had methodically endeavoured to investigate the problem on the spot, by patiently studying the complex agencies at work on this atoll, by carefully inquiring into the changes of the past, and by interpreting through their aid the processes of the present, we should be obliged to answer: "Scarcely at all. A theory advanced on the very threshold of such an investigation explained so well our limited knowledge by a movement of subsidence, that it is only of late years that doubts have arisen and that a new theory has been advanced opening up the lines of research to which you refer." H. B. GUPPY.

"Bambangala."

I SHALL be glad if you will allow me to call the attention of those who visit the Congo Free State to the curious antelope called "*Bambangala*," which is spoken of by Captain Bateman in his "The First Ascent of the Kasai," lately published by Messrs. George Philip and Son. Captain Bateman describes it as being "in size as large as a mule; of a bright chestnut colour, striped with creamy white, much in the manner of a zebra, on the back and sides, and dappled on the neck and flanks."



From the form of the horns shown in the figure (which by the kind permission of Messrs. George Philip and Son, is here reproduced from Captain Bateman's book), it would appear that this antelope must belong to the genus *Tragelaphus*, but probably to a new species.

I should be very glad to examine specimens of the head and horns of this antelope, in case any of your correspondents in the Congo should meet with it, or to have some further information on the subject from those who have visited that region.

P. L. SCLATER.

Zoological Society of London, 3 Hanover Square,
London, W., May 7.

Inclusion of the Foot in the Abdominal Cavity of a Duckling.

A DUCKLING, four days old, sent from Eastry in Kent, hatched on April 25, 1889, presented the above curious abnormality. The right lower extremity was normal in every respect; the left appeared on superficial examination to be absent. An incision made through the skin over the left flank at once disclosed the left limb, the joints being flexed to their utmost extent, and the thigh adducted, so that it lay in contact with the abdominal muscles of the left side; at the tibio-tarsal joint the limb passed through the wall of the abdomen, a few millimetres above the symphysis pubis. A portion of the yolk-sac protruded at the aperture by which the foot penetrated the abdominal wall. Opening the abdomen showed the remainder of the yolk-sac, its connection with the ileum, and the left foot lying upon the surface of the intestines. This included foot was fully developed, having a complete web, and being covered with scales. An adhesion existed between the outer surface of the yolk-sac and the left leg in the region of its tibio-tarsal joint, and there were also adhesions of the sac to the skin of the abdomen,

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